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Note on some Points connected with the Determination of the Coefficient of the Parallactic Inequality. By E. J. Stone, Esq.

There are three points raised by Messrs. Campbell and Neison in their paper "On the Parallactic Inequality" (*Monthly Notices*, vol. xli. No. 5), to which I should wish briefly to reply.

1. The correction 1″·56, which I have applied to the coefficient of the parallactic inequality deduced directly from a comparison between the Greenwich observations and Burckhardt's longitudes of the Moon, is the mean correction which is supposed to be required for errors in Burckhardt's tables when observations extending over nine years, and thus including a revolution of the apse, are grouped together; and Burckhardt's tables were used by me for the determination of the coefficient of the parallactic inequality in groups of nine-year periods. If Messrs. Campbell and Neison could show that this correction was in error 0″·5, for such a group of observations, they would have shown that the resulting value of the solar parallax was in error between 0″·03 and 0″·04. But these gentlemen have shown nothing of the kind; and I feel certain that no error, to this extent, has been committed through the neglect of any recognised lunar inequalities. I believe the correction applied was more than sufficiently accurate for the objects which I had in view. I did not expect to fix the value of the coefficient of the parallactic inequality, with the materials available in 1867, within 0″·25, and, although the result which Messrs. Campbell and Neison have recently found agrees very closely with that which I found in 1867, I am still very doubtful whether we do, as a matter of fact, know the value of the coefficient within a quarter of a second of arc.

2. I am afraid that Messrs. Campbell and Neison have hardly appreciated the force of the objection which I raised, and I think substantiated, against the logical completeness of their investigation in the *Monthly Notices*, vol. xl. Nos. 7 and 8.

I have, at present, seen no proof that the differences, between the optical semi-diameters of the Moon near the Full and Quarters, in the winter and summer months respectively, bear to each other a ratio of 0·25 to 0·75, or 1 : 3, rather than a ratio of 1 : 4, or 3 : 8; or 1 : 2. But waiving this point: what then? The investigation of Messrs. Campbell and Neison led them to the values of I, the supposed constant of irradiation, which I have given in the *Monthly Notices*, Nov. 1880, vol. xli. No. 1. These values of I are very nearly, indeed, as discordant as the values deduced from an investigation which Messrs. Campbell and Neison have most strongly condemned, simply on account of the discordant values of I to which it led. It appears to me, therefore, that the authors might with equal justice, and on the same grounds, have condemned their own investigation; and I entirely fail to see how

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they have vindicated their investigation from the demurrer raised against it.

There is no doubt whatever that when we possess a perfect Lunar Theory, with all the six constants accurately determined, and are in possession of perfect methods for the reduction of our observations, including the elimination of all systematic errors, instrumental and personal, we shall free our equations of conditions from all discordances; all I maintain is that Messrs. Campbell and Neison have not yet succeeded in the elimination of such discordances, and that, as a matter of fact, their attempts to do so in this matter of irradiation have not, according to their own standard, been successful.

3. I am afraid that I must take exception to the statement made by Messrs. Campbell and Neison that "the entire series of the Greenwich meridian observations of the Moon unite in supporting the hypothesis of a forty-five-year inequality." To support this statement, the following results, as coefficients of the parallactic inequality corrected for the forty-five-year inequality, are given:—

"From observations

1851–1858	$124''\cdot23$
1862–1869	$124\cdot09$
1870–1876	$124\cdot13$.

"Airy's value

1751–1846	$124''\cdot13$."

These results undoubtedly strongly support the statement made; but the evidence brought forward will not, I fear, bear examination. The division of the group of observations 1862–1876, adopted by the authors, can afford no independent evidence for or against the forty-five-year inequality. But if we divide the observations into two groups of seven years each, with corrections for the inequality above the mean correction for the whole group, $1''\cdot02$, and equal to or below that mean, we shall find for the first group a value of the coefficient $125''\cdot07$, and for the second a value of $125''\cdot20$. These two results only differ by $0''\cdot13$, but if we introduce the correction for the supposed forty-five-year inequality, the results will be found to differ by $0''\cdot39$.

So far, therefore, as the results given by Messrs. Campbell and Neison for the values of the parallactic inequality can be trusted to afford evidence in seven-year groups, upon questions affecting a quarter of a second of arc, these results show that the value of the parallactic inequality found from the observations about 1870 does not require to be diminished by $1''\cdot02$. And, with respect to the results given for the observations 1851–1858, the authors have applied a correction for semi-diameter deduced as a correction to Hansen's semi-diameter, and

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have apparently overlooked the fact that the observations 1851–1858 were either reduced directly with Adams' value of the semi-diameter, or corrected by the Astronomer Royal for the differences between the adopted values and Adams' value of the semi-diameter. As Adams' value is quite half a second larger than Hansen's value, the coefficient found by the authors requires to be increased from this cause by half a second. Nor is this all. In the paper, vol. xl. 1880, May and June, Nos. 7 and 8, the authors have increased the value of the parallactic inequality for error of Hansen's semi-diameter for the Transit Circle observations by 0″·29 in the mean group, whilst in the case of the observations 1851–1858 they have decreased the coefficient by 0″·18. These changes are certainly not satisfactory. So far as the observations 1854–1858 are concerned, which were made with the chronographic method, the same mean correction should be applied. And from a direct investigation I find that the correction required for error of adopted semi-diameter is 0″·79; which agrees very closely with the result above indicated, viz.:—

Excess of Adams over Hansen ...	+0″·52
Correction to Hansen	0·29
	0·81

It appears to me that accepting all the authors' results except the correction to semi-diameter—which is certainly not satisfactory—the coefficient of the parallactic inequality from the observations 1851–1858, instead of agreeing with the result 1862–1876 after correction for the forty-five-year inequality is greater than that result by one second of arc, and is made more discordant by the application of the hypothetical inequality.

The value given for the earlier observations 1751–1846 (viz. 124″·13) is also to be received with caution.

The observations 1750–1810 give a value about 121″·73, whilst those from 1811–1851 give a value of 124″·37. The authors bridge over the difficulty presented by the large difference between these results by assuming that all the earlier observations require a constant correction of 1″·6; and this although there were several changes of instruments, observers, and methods of observing. Nor is this all. To reconcile the result thus obtained by this constant correction and the forty-five-year inequality, they have to assume that the result for the group of observations 1843–1851 requires to be diminished by 1″·78 for supposed errors of diameter. I cannot recognise the legitimacy of these steps. That the earlier observations, as reduced, show the existence of some systematic discordance with a period of about forty-five years was pointed out many years ago by the Astronomer Royal; but that such a discordance is due to an inequality in the theoretical determination of the Moon's longitude $=1''\cdot 20 \sin [D-8(y-1825\cdot 5)]$, appears to me most doubtful,